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DR. J. W. SHIPLEY, who during the last two years has been assistant professor of analytical chemistry at the Ohio State University, is going to the Agricultural College of the University of Manitoba, Winnipeg, as assistant professor of chemistry.

MR. F. S. NOWLAN, of Columbia University, has been appointed instructor in mathematics at the Carnegie School of Technology, Pittsburgh, Pa.

AT Lehigh University, R. L. Spencer has been promoted to be assistant professor of mechanical engineering and S. J. Thomas to be assistant professor of biology.

DISCUSSION AND CORRESPONDENCE

ATMOSPHERIC TRANSMISSION

TO THE EDITOR OF SCIENCE: Replying to the first point in Mr. Abbot's communication in SCIENCE for February 18, 1916, page 240, in reference to the variability of atmospheric transmission of solar radiation during a single day, I have never denied that occasions may be found when the diurnal transmission is substantially constant, but have distinctly averred that such uniformity sometimes exists. What I must deny, however, is that the Mount Wilson observations of September 20 and September 21, 1914, are in the category of measurements unaffected by diurnal changes of transmissivity. The trifling variations from minute to minute on these dates may indeed have been small, but these are not now in question. They may be eliminated for our purpose by passing a mean curve through the plotted observations; but when thus smoothed, the mean curve shows peculiarities which can not be neglected. I have drawn such curves and find the following significant features:

Concerning ourselves simply with the transmission of solar radiation by a unit of atmospheric mass, equivalent to a single vertical transmission, if the rays presented for transmission were of unvarying quality, and if the transmissive properties of the atmosphere remained likewise unchanged through the day, we should have a perfect day for the purpose of the deduction of the solar constant from a comparison of high-sun with low-sun meas-

ures. But, in general, neither of these desiderata exist. For example, on September 20, 1914, between air masses 2 and 3, the radiation fell off from 1.437 to 1.311. Transmission by unit mass,

$$T_{(2-3)} = 1.311/1.437 = 0.9124.$$

Between air masses 7 and 8, the radiation diminished from 0.983 to 0.922.

$$T_{(7-8)} = 0.9378.$$

Here it is as if the air had become more transmissive, although this undoubtedly means that, for one thing, the rays which have penetrated more deeply have become more transmissible through the total loss of some of their more absorbable ingredients. Be this as it may, we can not discriminate between this source of variability and another one which is always present (and always potent except in times of extreme cold) and which comes from the evaporation of water at the earth's surface and the ascent of considerable masses of aqueous vapor into the convectional layer of air *in the middle of the day*, whereby the midday atmosphere becomes less transmissive, and the apparent transmission deduced from comparison of high-sun with low observations is illusory.

For air masses 14 and 15, the radiation was 0.680 and 0.648; $T_{(14-15)} = 0.9530$. That is, there was still a further increase of transmissivity of unit air mass with this larger departure from midday conditions. Similar results are found on September 21, 1914, namely,

$$T_{(2-3)} = 1.297/1.437 = 0.9028,$$

$$T_{(7-8)} = 0.889/0.947 = 0.9390,$$

$$T_{(14-15)} = 0.630/0.660 = 0.9545.$$

M. R. Savélief, observing in Russia in very cold weather, obtained between air masses 4.5 and 5.5 a transmission equivalent to that for Mount Wilson between air masses 2 and 3, and was able to match Mount Wilson $T_{(7-8)}$ with the interval between air masses 9 and 10. His observations represent a much closer approach to uniform transmission than those cited by Mr. Abbot; and this is doubtless due to the comparative absence of aqueous vapor whose pressure at the earth's surface was from 0.7 to 0.9 mm. in the Russian measures, whereas the Mount Wilson observations were made with

pressures of water vapor varying between 4.62 and 9.99 mm. on September 20, and between 2.21 and 7.49 mm. on September 21. The total quantity of precipitable water in the atmosphere on September 20, as determined by Fowle's spectroscopic method, varied between 3.32 at low-sun observations to 8.6 mm. at high-sun observations, and on September 21 between 3.8 and 8.3 mm. Thus there was between two and three times as much water vapor present in the midday air as there was at low-sun observations. Since the transmissivity of the atmosphere is known to diminish with the increase of aqueous vapor, other things remaining equal, would it be at all likely that Mr. Abbot's assertion that the transmissive quality of the atmosphere above Mount Wilson remained unchanged throughout these days, should turn out to be true? And do not the partial transmissions which I have derived from his own figures point to a contrary conclusion?

In his second paragraph, Mr. Abbot tries to discredit my measurements of the distribution of intensity in the spectrum of the earth-shine, because my statement that the night sky at Flagstaff in the early morning of August 9 and 10, 1912 (civil reckoning), was exceptionally clear, appears to him incompatible with the experience of himself and others that the "skylight near the sun in daytime notably increased" during that month. My statement rests upon the following evidence:

The spectrograms of the earth-shine were made for me at Dr. Lowell's observatory by Dr. V. M. Slipher. I had asked Dr. Slipher to place the slit of his spectroscope half on and half off the dark limb of the moon. In this way there were obtained juxtaposed spectrograms of precisely the same duration of exposure and photographic development, one of the earth-shine *plus* diffuse skylight from intervening air, illuminated by the light passing through it from the bright crescent of the moon, and the other of the skylight alone, from which the true earth-shine was obtained by difference. Dr. Slipher had given me his impression from eye estimate that the sky on August 8 (astronomical date) was "good," and

on August 9 "excellent"; but my quantitative measurements are far superior to any eye estimates, and these tell the following story:

Without going into the minutiae of the photographic corrections, I will merely record that all necessary corrections of this sort have been applied. Those interested will find the details given in my paper on "The Photographic Spectrography of the Earth-shine and a Spectrophotometric Comparison of the Earth-shine with Moonlight, Skylight and Sunlight, together with a Study of the Difficulties of Photographic Comparisons."¹

The ratios of exposure durations for earth-shine (t_E) and for moonlight (t_M) were

$$\begin{aligned} \text{August 8, 1912, } t_E : t_M &= 4800 : 1, \\ \text{August 9, 1912, } t_E : t_M &= 2840 : 1. \end{aligned}$$

The average of the ratios of photographic opacities on the spectrograms for earth-shine and moon (J_E/J_M) and for earth-shine and sky (J_E/J_S) were

$$\begin{aligned} \text{August 8, 1912, } J_E/J_M &= 1.360 : 1; J_E/J_S = 3.62 : 1, \\ \text{August 9, 1912, } J_E/J_M &= 1.062 : 1; J_E/J_S = 8.49 : 1. \end{aligned}$$

The ratios of moonlight to the skylight just outside of the extreme border of the moon's dark limb were therefore

$$\text{August 8, 1912, } \frac{t_E}{t_M} \times \frac{J_M}{J_E} \times \frac{J_E}{J_S} = \frac{4800 \times 3.62}{1.360} = 12,776 : 1,$$

$$\text{August 9, 1912, } \frac{t_E}{t_M} \times \frac{J_M}{J_E} \times \frac{J_E}{J_S} = \frac{2840 \times 8.49}{1.062} = 22,704 : 1.$$

For comparison I give corresponding values of the ratio of moonlight to skylight, obtained at Westwood, Massachusetts, during my visual measures of the earth-shine, which give an idea of the variation which is to be anticipated in skies ordinarily reputed "clear": 1911. Sept. 28, 52:1 (sky hazy); Sept. 30, 3095:1 (clear); Oct. 2, 1149:1 (clear, followed by cirro-stratus); Oct. 26, 3033:1 (clear); Oct. 29, 3626:1 (clear); Nov. 16 (A.M.), 1871:1 (clear); Nov. 17 (A.M.), 8579:1 (exceptionally clear); Nov. 27, 1358:1 (clear to hazy); Dec. 14 (A.M.), 9380:1 (exceptionally clear). 1912. Feb. 20, 2476:1 (faint cirrus bars).

Here the greatest degree of clearness at this station about 200 feet above sea level, gave a

¹ *Astronomische Nachrichten*, Nr. 4819-20, November, 1915.

ratio of not over 10,000:1, which falls considerably short of the Flagstaff conditions on either of the given dates.

It seems to me that I am fully justified in calling the mornings of August 9 and 10, 1912 (civil date), exceptionally clear, even for Flagstaff; and I submit that exact quantitative measurements, such as I have given, are to be preferred to Mr. Abbot's vague estimate that "skylight near the sun in daytime notably increased." If the discrepancy is regarded as sufficiently noteworthy, I would suggest that it indicates that the "dust cloud from Katmai" was not as universal as Mr. Abbot supposes. Mr. Abbot has inferred from the consistent agreement of his observations with those of some other observers, that the obscuration which he attributes to the eruption of Katmai was world-wide and continuous; but this is a mere hypothetical conjecture, in the absence of anything known to the contrary, which a single good opposing observation can overthrow.

While the presence of a clear and uniform sky is an advantage in such delicate measures as those of the spectrum of the earth-shine, it is not an indispensable one, because my method of observation permits accurate measurement of and correction for the interfering skylight; and it is not quite exact to say that "Mr. Very hangs the merit of his work on the exceptional clearness of August 8 and 9, 1912," because I have given these observations no greater weight in the final result than is assigned to other dates when the skylight was considerably stronger than the earth-shine. Being freed from the variable effect of skylight, my measures are sufficiently exact to show not only the variation of the earth-shine from day to day with the changing phase of the illuminating earth, but they also detect variations in the quality of the light which are attributable to a variable proportion of blue "skylight," *i. e.*, sunlight scattered upward by the clear air in the same way that skylight is scattered downwards, and varying in amount according to the cloudiness of the earth's hemisphere facing the moon.

Coming to Mr. Abbot's third point, in which

he defends the conclusions of Mr. A. Ångström, who finds a mean atmospheric transmission of terrestrial radiation by clear air of about 15 per cent., where I obtain about 40 per cent., I anticipated Mr. Ångström's curve of instrumental radiation to limited areas of sky at different zenith distances, and obtained a similar, but more accurate curve;² but I did not make his mistake of confounding this purely instrumental result with the radiation of the earth's surface to outer space. It is true that the radiation from a small surface so circumscribed that the rays can only escape through a narrow aperture, pointing to the sky in a direction but little elevated above the horizon, so that the path through the lower moisture-bearing layers of the atmosphere is equivalent to a passage through a considerable depth of water, is usually so impeded that scarcely any gets through. But the radiation of the indefinitely extended surface of the earth, free to radiate vertically through a comparatively shallow layer of moist air, escapes readily. For such radiation there is an extensive region of the spectrum between 8.5 and 12.8 μ , where the transmission averages something like 80 per cent. Yet even the maxima, or spectral regions of comparatively free transmission, are almost obliterated in the long road through the air in a pointing not much above the horizon. This is an important fact, and its explanation has seemed to me to lie in the presence of multitudes of excessively faint absorption lines in the parts of the spectrum where the maxima reside—lines which are too fine and too faint to be individually discriminated by the bolometer, but which increase in intensity and finally produce a somewhat general obscuration of the spectrum, even in its more transmissible portions, when the air path becomes excessive. The recognition of the existence of these faint lines by Mr. Abbot would go a long way towards removing the discrepancy between our points of view.

I will not trespass on your space to point out the numerous errors in Mr. Ångström's

² See my paper, "Sky Radiation and the Isothermal Layer," *Am. Jour. Sci.*, Vol. XXXV., Fig. 2, p. 383, April, 1913.

argument, since my paper on "Fundamental Distinctions Special to the Process of Transmission of Terrestrial Radiation by the Atmosphere, and the Value which is obtained for the Coefficient of Transmission when these are considered" will appear in full in the *American Journal of Science*. [The paper has since been published in the issue for June, 1916, Vol. XLI., pp. 513-521.] FRANK W. VERY

WESTWOOD ASTROPHYSICAL OBSERVATORY,
February 22, 1916

SOME NOTES ON THE OLYMPIC PENINSULA,
WASHINGTON. A REPLY TO CRITICISMS
BY ARNOLD AND HANNIBAL

IN "The Marine Tertiary Stratigraphy of the North Pacific Coast" by Ralph Arnold and Harold Hannibal, page 604,¹ is this paragraph:

A. B. Reagan, 1908, "Some Notes on the Olympic Peninsula." Most of the geological data in this paper are adopted from one by the senior writer (Arnold) mentioned. . . . The description of the Quillayute formation is based on the glacial filling of the valley of the Quillayute River. If Reagan had visited the locality from which the fossils described from the Quillayute (formation) were brought by Indians, he would have found it to be about two miles from Devil's Club Swamp where he says they occur, and the formation lithologically very different from what he describes. It is typical Empire formation.

Mr. Arnold's article that he says my work was adopted from is "Geological Reconnaissance of the Coast of the Olympic Peninsula, Washington,"² totalling 18 pages; my cited article, "Some Notes on the Olympic Peninsula," covers 108 pages besides plates.

I visited the region and collected the fossils described myself, with the exception of the fossil *Ranella marshalli*, which was given me by Mr. Marshall, as is stated in the article. I made a good many trips to the place both with Indians and whites. We went both by canoe up the river and also on foot in from Quillayute Prairie. James Clark, now county commissioner of Clallam County, Washington, accompanied me on my first trip; George Woodrough, now of Ilwaco, Washington, was with

¹ Reprint from *Proceedings of the American Philosophical Society*, Volume LII., No. 212, November-December, 1913.

² *Bull. Geol. Soc. America*, Vol. 17, pp. 451-462.

me on another trip. On practically all the trips I crossed the Devil's Club Swamp from the bend in the river to the bluffs adjacent and north of where Maxfield Creek entered Quillayute River when that river ran against the western bluffs, instead of about a half mile eastward as it does now (at the old mouth of Maxfield Creek—not a later mouth of that creek). No fossils were collected in the Devil's Club Swamp; the article is very plain on this point, that the fossils were collected in the bluffs west of the old mouth of Maxfield Creek (that is, from near the present mouth northward along the bluffs).

I will now quote from page 203 of my cited article:

Quillayute Formation.—(This is under the general heading "Pliocene," on page 202.) This formation occupies the valley of the Quillayute River and the country drained by its western tributaries at least to their respective middle courses. . . . The boundaries of the formation were not determined. In the interior region, where exposed along the Bogachiel River, it is composed of sandstone and bluish shale; the coast exposures are all conglomerates or a coarse, gravelly rock resting unconformably upon the older rocks exposed there. The base of the formation was not seen, consequently was not ascertained. The sandstone series was found to be extremely fossiliferous, and in it the fossils are beautifully preserved. Fossils were found in two horizons—in the north bank of the Bogachiel River in a bluish gray rock in section 22, township 28 north, range 14 west of the Willamette meridian, and in the bluff south of the abandoned channel of Maxfield Creek on the south side of the Bogachiel River, in sections 28 and 29 of the township and range above. But fossils were obtained only from the latter location, as the former was below the surface of the water at the time visited. Below is a description of the fossils obtained.

Fossils of the Quillayute Formation—Lower? Pliocene, exposed in the Vicinity of Quillayute, Washington:

Here follows a two-page comparison of the Quillayute-formation fossils with the fossils of other regions, with the final conclusion (page 206) that:

Consequently, this (the comparison results) would seem to place the formation at the bottom